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Informational Barriers to Credit for Migrants: Evidence from Guatemala

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I. Introduction

A well-documented stream of literature suggests that informational issues surrounding an individual's repayment history can lead to substantial rationing in formal credit markets. In many developing countries that lack robust contract enforcement and national information-sharing institutions, credit markets depend on local information and reputation to bolster repayment incentives. In such contexts, migration across locations reduces the amount of information available about an individual in her destination. Moreover, individuals can escape damaging information by switching to locations where their past actions are less well known. Migrants thus offer a unique set of potential borrowers who are particularly informationally opaque and among whom strategic default may be particularly problematic. Thus, we might expect to find exacerbated credit rationing among individuals who have migrated in such contexts.

Few studies have examined migrants under this lens. Eliana La Ferrara (2003) shows that among Ghanaian villagers, migrants are much less likely to obtain loans from kinsmen, who are particularly reliant on reputation and generalized reciprocation in enforcing loan repayment, than from other sources such as banks or cooperatives. Given her focus on informal borrowing among social groups, La Ferrara does not address to what extent informational and enforcement issues differentially affect migrants when they seek loans in the formal credit market, where reciprocity is not generally at issue. In a different context, Guiso, Sapienza, and Zingales (2004) find that migrants from the south of Italy to its north are less likely to use checks or hold stocks, which they interpret as being related to the southerners' lower level of social capital. However, their paper does not find any causal effect of migration per se when

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controlling for the social capital in one's birthplace and does not seriously address the potential for reverse causality and omitted variable biases.

Do migrants in developing countries face exacerbated informational barriers to access to credit? I answer this question using data on individual migration and credit holdings in Guatemala from 2000. As McKenzie and Sasin (2007) highlight, assessing the impact of migration on various socioeconomic outcomes is complicated by endogeneity, self-selection, and omitted variable biases. To address the impact of migration on credit access, I adopt a novel instrumental variables approach using the broad thematic coverage of the World Bank's Living Standards Measurement Survey (LSMS), which includes questions about credit use, migration histories, and perceptions and experiences with violence and crime. Given that migration within Guatemala has been driven in part by patterns of violence within the country (Morrison 1993), I use the LSMS to identify the individuals who are most concerned about violence or crime in their local communities. These individuals are most likely to be sensitive to the actual rates of violence in the areas in which they were born and to move away from particularly violent areas. This exogenous migration propensity can be used to study the reduced access to credit associated with migration. Because migration due to violence in one's birthplace is essentially a one-time move, such migrants may be no more likely to default on their future loans than natives of an area. However, lenders are not likely to observe an individual's sensitivity to violence or crime and thus treat such individuals as they do all migrants. If these individuals experience less access to credit than natives do, the matter becomes a noteworthy economic inefficiency.

The setting I study offers a compelling institutional environment: Guatemala saw significant internal (and international) migration in the years preceding the 2000 survey, much of it driven by violence. Creditor rights have historically been weakly protected in Guatemala, and in 2002 a World Bank appraisal cited "poor judicial procedures for rapid debt collection" and an "inadequate legal framework for secured transactions" (2002, 2) as key problems affecting the financial sector. Moreover, there was little information sharing among banks, cooperatives, or other lenders in Guatemala prior to 2000. TransUnion opened the first credit bureau in the country in 2001, and credit information–sharing mechanisms among microfinance institutions began appearing in late 2002. Thus, looking at Guatemalan survey data from 2000 offers an opportunity to assess the effects of migration on formal credit in the absence of well-developed credit information sharing and strong creditor protections. This is exactly the setting in which one would expect to find substantial evidence of credit rationing affecting migrants.

The difficulty migrants may have in signaling their creditworthiness to

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lenders at their destinations should be understood in the broader context of the many varied links between migration and credit, income, and welfare. Bryan, Chowdhury, and Mobarak (2010) provide new evidence that randomized access to small loans used to cover seasonal migration costs can lead to substantially higher income and consumption levels in drought-prone areas of Bangladesh. Looking at international migration, Clemens (2010) finds that Indians randomly granted temporary work visas in the United States earned significantly higher wages than those who were not granted these visas and thus either remained in India or migrated elsewhere.

More broadly, internal migration is an important mechanism through which a country's human resources are aligned with their most productive uses. As countries grow economically, they often transition from regionally segmented markets to integrated ones in which both goods and people move across space. These and other channels through which migration, credit, and income interact often lead to situations in which the ability to migrate can make people in developing countries better off. The findings presented herein suggest that, at the same time, migrants may face exacerbated informational barriers in accessing credit. These barriers reflect an important economic inefficiency and provide support for policies emphasizing the introduction of information-sharing mechanisms in developing countries and improvement and expansion of existing ones, particularly as societies experience greater mobility.

The article is laid out as follows: In Section II, I discuss the existing literature on migration and access to credit. I then present an overview of a theoretical model in Section III, illustrating how mobility can reduce access to credit in the presence of weak contract enforcement (the details of the model are included in an appendix at http://research.economics.unsw.edu.au/abenyishay/Papers/Online%20Appendix.pdf). Turning to the empirical evidence, I lay out an identification strategy in Section IV, followed by a discussion of the data in Section V. I offer results in Section VI and test the validity of the exclusion restriction in Section VII. I consider alternative explanations for these findings in Section VIII. In Section IX, I conduct a series of additional robustness checks. Finally, I present conclusions in Section X.

II. Existing Literature

An extensive literature has focused on explaining the lack of access to credit in developing countries. This literature largely focuses on imperfect enforcement and informational asymmetries, founded in large part on Stiglitz and Weiss's (1981) theoretical work demonstrating how adverse selection and moral hazard can cause backward-bending supply curves and redlining of potential borrowers (see Conning and Udry [2005] for a survey of these issues as applied

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to agricultural settings). More recent papers have focused on empirically identifying the nature and scope of the specific frictions. Karlan and Zinman (2009), for example, find that adverse selection and moral hazard lead to default rates as high as 20% in an experimental setting. As Ghosh, Mookherjee, and Ray (2000) note, imperfect enforcement can also be problematic, particularly in developing country contexts. Imperfect enforcement refers to situations in which frictions arise from voluntary default. However, individual mobility is rarely cited as a cause of any of these frictions. In explaining the widespread belief that female borrowers of microfinance institutions are less likely to engage in moral hazard, Armendariz de Aghion and Morduch (2005) suggest that greater immobility may make default more costly for female borrowers and monitoring cheaper for their lenders. Nonetheless, this suggestion that individual mobility may be a source of friction in credit markets has not been extensively investigated.

As previously noted, one study that does explicitly link migration and credit access is La Ferrara (2003), which finds that migrants are less likely to receive credit from kin group networks than from other sources, such as moneylenders, banks, or cooperatives. However, because it estimates the likelihood that a given loan will come from each source, this study does not identify the extent to which migrants are less likely to receive loans from any source at all. Moreover, La Ferrara finds that reciprocity as an enforcement mechanism appears to dominate informational asymmetries in explaining informal lending among kin groups, which is not necessarily the case among banks and other formal lenders.

Another study that considers migration in a different context is Guiso et al. (2004). In their study of the effects of social capital on the use of financial instruments such as checks and stocks in Italy, the authors assess to what degree this behavior changes when an individual moves to a new region of the country. In ordinary least squares (OLS) regressions, they find that migrants from the south to the north of Italy are less likely to use these instruments, presumably because of the southerners' lower level of social capital. However, they do not find significant effects of migration per se, which is perhaps not surprising given the confounding effects of reverse causation and omitted variables. Moreover, Guiso et al. focus largely on the differential use of rather than access to credit among these movers.

One mechanism through which credit rationing can be mitigated is relationship lending, as Petersen and Rajan (1994) highlight. By building a relationship with a borrower over an extended time period, a lender becomes better informed about the borrower's riskiness and thus can overcome moral hazard, adverse selection, and contract enforcement issues. Migration to a new

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location often represents a severing of those relationships, making migrants even more opaque relative to the average native, who may have preserved his existing relationships with lenders.

Credit access issues specifically among Guatemalan households have been studied from a number of angles by Bruce Wydick and a series of coauthors. Wydick (2002) tracks 239 clients of a microfinance institution in western Guatemala, finding differing enterprise growth for women and men due in part to gender differences in credit constraints on consumption and investment. Wydick, Karp, and Hilliker (2008) identify substantial social imitation in obtaining credit among similar households, often through church and neighborhood networks. Wydick (1999) suggests that social repayment enforcement may also be particularly potent in this setting. Moreover, Luoto, McIntosh, and Wydick's (2007) study of the credit information system implemented among Guatemalan microfinance organizations beginning in 2002 finds a large positive impact on loan repayment. Although they have yet to address whether the significant migration in Guatemala has been a source of credit market frictions, these studies suggest that there is significant scope for both adverse selection and moral hazard among rural Guatemalan borrowers.

A much broader stream of literature has analyzed the self-selection of migrants in terms of key unobserved characteristics, with Borjas (1987) serving as a seminal paper. In these models, the information structure causing these characteristics to be unobserved is generally taken as given. As Lucas (1997) suggests, relatively little empirical work has addressed whether the act of migration actually obscures information about the individual migrant and thus in some instances negatively influences outcomes at the destination (Lucas highlights employment outcomes). This issue is particularly crucial for credit markets because information can be used to counteract weak contract enforcement. Moreover, Chiswick (1999) suggests that any favorable self-selectivity among migrants would be expected to be weaker among those who move primarily on the basis of factors other than their own labor market or entrepreneurial success, including those who move to "escape from real or perceived threats to their freedom or safety" (184). These findings suggest that studying migrants whose propensity to migrate is more related to local violence and crime as well as their sensitivity to personal safety offers a useful setting to study frictions such as weak contract enforcement and information sharing.

Finally, this article also builds on a literature examining empirically the effects of migration on various socioeconomic outcomes, though one that has faced limitations in identifying the effects of within-country migration (McKenzie and Sasin [2007] provide a useful review). Bryan et al. (2010) use a field experiment to study the effect of access to small loans used to cover

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seasonal migration costs on postmigration income and consumption levels in drought-prone areas of Bangladesh. The authors find that those who received these loans and migrated were substantially more likely to enjoy higher consumption levels, even after repeating these seasonal migration paths in subsequent years. Because these types of experiments involving migration are relatively rare,[1] the existing literature has primarily relied on instrumental variables. These studies primarily look at one-directional migration from sending to receiving areas, using the characteristics of the sending areas to identify effects on individuals in their destinations (or vice versa). Munshi (2003), for example, uses rainfall in Mexican villages as an instrument for migration when looking at outcomes abroad, and Hildebrandt and McKenzie (2005) and McKenzie and Rappaport (2007) use historic Mexican state-level migration and railroad development proxies. These papers do not identify the effects of multidirectional migration within a country, in which each region both sends and receives migrants, because the region-specific shocks used as an instrument for some individuals could directly affect the outcome of interest. Similarly, McKenzie et al. (2006) look at distance from the New Zealand consulate in Tonga as an instrument for migration to New Zealand, though such variables could similarly be correlated with home region characteristics. Beaudouin (2005), Acosta (2006), and Mansuri (2006) use indicators of migration networks and history at the village or household level as instruments, though McKenzie et al. (2006) provide evidence that such networks can be poor instruments because they can directly affect outcomes of interest. More recently, Beegle, De Weert, and Dercon (2011) look within the household and identify those individuals whose age rank makes them most likely to migrate (in addition to rainfall shocks), though these authors make use of rare panel data in which they found and recontacted Tanzanian families after more than 10 years. In contributing to this literature, this article develops an instrumental variable (IV) approach to estimate the effects of within-country migration using a single cross section of data.

III. Theory

As previously noted, theoretical explanations of credit rationing in developing countries typically highlight asymmetric information and contract enforcement problems. These can be thought of separately as issues involving involuntary and voluntary default, respectively. Asymmetric information problems, such

[1] McKenzie, Gibson, and Stillman (2006) present one example of a lottery for migration from Tonga to New Zealand. Comparing alternative strategies against this experimental benchmark, they find that a good instrument produces the least amount of bias (9%), as opposed to propensity score matching (19%–33%), double difference (20%), and OLS (31%).

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as adverse selection and moral hazard, involve involuntary default when borrowers are unable to repay their loans. Models explaining rationing due to involuntary default assume that contract enforcement compels borrowers to repay their loans when they are able to do so. In many less developed countries, however, contract enforcement is generally problematic, and debt contracts may be particularly challenging to enforce. Thus, we can think of voluntary default arising out of poor contract enforcement as a more basic economic friction.[2] In the empirical section, I assess the evidence on the importance of contract enforcement relative to involuntary default in the Guatemalan context. In this section, I describe a theoretical model of voluntary default when individual borrowers are able to migrate. For brevity's sake, I outline the model's main features and results in narrative form, with detailed mathematical notation included in the appendix.

Although much work on voluntary default has focused on sovereign governments (Eaton and Gersovitz 1981), Ghosh et al. (2000) show that an individual borrower's ability to default strategically can also affect within-country credit markets. In such contexts, information sharing and implicit cooperation among lenders can sometimes reduce voluntary default by incentivizing borrowers to repay loans to avoid exclusion from future borrowing. That is, lenders may share information on a borrower's repayment history and follow a trigger strategy that rejects loan applicants with prior defaults. Ghosh et al. allow this information-sharing technology to vary in quality and show that a more effective technology—one with better detection of past defaults—can lead to a more efficient outcome.

I extend the Ghosh et al. model in two key ways: First, I incorporate information dissipation over space. The intuition is that information sharing among lenders may be weaker when the lenders are located in different towns than when they are located in the same town. Second, I extend the Ghosh et al. model by incorporating heterogeneity in individuals' migration costs, which include both a permanent and a time-varying component. In essence, some particularly mobile entrepreneurs with low permanent migration costs are

[2] This is not to say that informational asymmetry problems may not still be present after contract enforcement is suitably strengthened. For example, on the basis of Stiglitz and Weiss's (1981) seminal paper on adverse selection, a credit market could exhibit greater rationing of migrants if lenders "know less" about individual migrants' returns than they do about individual natives. Different risk formulations, however, have been shown to lead to overlending rather than rationing (e.g., De Meza and Webb 1987), and the equilibrium outcome depends on the distributions of the different parameters. An extensive literature has also studied the scope for moral hazard in explaining credit rationing (see, e.g., Jaffee and Russell 1976; Aghion and Bolton 1997). This issue would particularly affect migrants when less is known about their choices of inputs for projects or, more generally, about their actions affecting their ability to repay loans.

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perennial flight risks, whereas other entrepreneurs may move because of a one-time shock. As lenders cannot observe these migration costs, they must form expectations about entrepreneurs' future migration and repayment strategies. In doing so, they lump migrants who are repeat defaulters together with those who may have stronger repayment prospects.

The model involves entrepreneurs who make individual repayment and migration decisions conditional on their expectation of lender rationing of migrants, whereas lenders make loan decisions conditional on the composition of repayment decisions among migrants. An equilibrium is reached when these lending decisions are rationalized by entrepreneur behavior. As borrower strategies are discrete choices, closed-form solutions for this equilibrium may not exist. In the appendix, I use simulations to show conditions on parameters under which it is not profit maximizing for lenders to extend credit to all migrants. Under these conditions, lenders will choose to ration loans to migrants in order to induce them to choose repayment of loans rather than default, as well as to reduce the incentive for future migration (since this creates a stock of more opaque applicants).

The model provides an explanation of equilibrium credit rationing for migrants. Because the set of indistinguishable migrants includes those who are likely to default and move repeatedly, lenders may reduce their loan exposure to all migrants, including those who plan to stay and repay their loans. Moreover, equilibrium rationing may emerge even if an individual lender would want to serve the marginal migrant because doing so would induce future migrants to default at higher rates. This is akin to "macro rationing" highlighted by Shapiro and Stiglitz (1984) as an explanation of a positive equilibrium unemployment rate. The model highlights the underlying issue that will be tested in the empirical section: there are some individuals who make one-time moves and are unlikely to move again, but because lenders cannot distinguish these from other migrants, they treat them as they do all migrants. Under some conditions, lenders will therefore not provide loans to these potentially creditworthy individuals because of their previous migration.

IV. Empirical Strategy

My theoretical model suggests that loans to migrants who move because of an exogenous factor (mobility shock) may be rationed, even if they would choose to stay and repay their loans in the future. I test this assertion empirically by identifying a factor that drives a major migration decision but is not likely to be otherwise correlated with credit demand or creditworthiness. To the extent that migrants who move for this exogenous reason receive less

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credit, this effect represents an economic inefficiency; individuals would receive loans if lenders could distinguish the reasons for their prior migration.

Testing this theory requires an appropriate institutional context in which enforcement of credit contracts is very weak and information sharing among lenders is limited and geographically concentrated. The situation in Guatemala in 2000 fits these features well. An assessment of the Guatemalan legal environment for credit transactions in the early 2000s found that obtaining judicial rulings on security interests took 1.5–2 years, on average (USAID 2004). Judicial mechanisms were also "susceptible to dilatory practices, including the interposition of *amparo* remedies by debtors alleging that their constitutional rights have been violated" (USAID 2004, VI-3).[3] Similarly, excessive delays were frequent and formalistic requirements were regarded as conducive to corruption. Even once rulings had been reached, there were no bailiffs or other private or public entities to enforce the rulings, and creditors at times resorted to extrajudicial enforcement, including vandalism, illegal repossession, and similar mechanisms. These were perceived as costly substitutes for efficient judicial enforcement.

The costliness and difficulty of contract enforcement in Guatemala led to the development of information-based alternatives to compel borrowers to repay loans. As previously mentioned, TransUnion launched the first private credit bureau in the country in 2001. In 2002, CREDIREF was founded as a credit bureau primarily serving hundreds of microfinance institutions and credit cooperatives, in addition to at least two major banks (de Janvry et al. 2003). Other collaborative efforts among microfinance institutions have been undertaken more recently. However, these information-sharing efforts were not formalized or offered on a national basis in 2000. Any information sharing taking place in 2000, therefore, was likely to occur through informal exchanges among loan officers or individual bank branches in particular geographic locations.

Moreover, in Guatemala, formal lenders could identify migrants through the documents each individual was required to produce as part of her loan application. Each individual's municipal identification card (*cédula de vecindad*) listed the issuing municipality and was generally required as part of loan applications. New identification cards from a migrant's destination municipality could certainly be obtained, and migrants who had resided in the destination for longer would be more likely to hold new cards. Nearly all the respondents in the 2000 survey indicate that they have an identification card,

[3] The *amparo* judicial procedure is intended to protect constitutional rights, but the lack of control against frivolous claims has led to abuses of this method (USAID 2004).

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and migrants are not more likely to have these cards than nonmigants (98.05% vs. 98.30%; t-statistic = 0.58). Lenders might also rely on physical, linguistic, and other differences in migrants' features, as well as their social networks and other personal contacts, to identify the length of time that individuals had resided in the municipality.

In this empirical analysis, it is important to distinguish "access to credit" from the observed use of credit, as nonusers of financial services often choose not to take out loans because they have no need for them ("voluntary self-exclusion"; World Bank 2008). I focus on access to formal credit in terms of traditional credit rationing, wherein lenders choose not to provide credit to some individuals even though these potential borrowers may be willing to pay the equilibrium interest rate ("involuntary exclusion"). That is, I estimate differences in the probability of applying for a loan, as well as the probability of holding a loan conditional on having applied for one.[4]

The empirical relationship between individual migration and access to credit is riddled with reverse causality and omitted variable issues. An individual could move in part because she expects better access to credit in her destination or she could be more entrepreneurial than her peers and thus less averse both to moving and to taking out a loan. Both of these reverse causality and omitted variable bias issues could lead to a positive relationship between migration and credit, confounding any actual rationing of loans to migrants. If we simply test the relationship between migration and credit access in an OLS regression, it might not be surprising were we to find no statistically meaningful relationship, given these confounding factors. I therefore adopt an IV approach that can better identify the impact of migrant status on credit supply by focusing specifically on those factors that affect migration but (1) are not observed by potential lenders and thus cannot directly affect the supply of credit to an individual and (2) are uncorrelated with individual unobservables that may be correlated with loan demand or lender perceptions of creditworthiness.

As previously noted, evidence on migration patterns in Guatemala suggests that a substantial portion of migration can be explained by the rate of violence in one's birthplace. The Guatemala LSMS survey is well suited to study this effect because it specifically includes questions that allow me to determine

[4] There are also reasons to believe that many individuals correctly expected to have their loan applications rejected and thus never applied. An alternative approach, used by La Ferrara (2003), instead controls for a variety of characteristics affecting the return to capital, such as the individual's age and education level, and interprets any residual effects of migration on credit holdings as being related to lender supply decisions. Using this alternative approach, I find results that are highly consistent with my preferred specification.

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which individuals are most likely to be sensitive to violence or crime in their local area. If some individuals are particularly sensitive to these issues (relative to the average individual in their community), they may be more likely to migrate away from violent and crime-ridden areas than would less sensitive individuals or those born into less violent areas. Lenders are unlikely to know how sensitive to such issues a potential borrower may be (conditional on the borrower's other characteristics). This sensitivity to violence or crime is likely to be driven by a variety of factors, including those that vary over a person's lifetime and those that remain constant, as well as those that vary with a person's location. Formally, we can think of the probability that an individual will be sensitive to violence or crime as

$$P_{ijkt} = z_i + X_i\Phi + \gamma f(t) + \mu_j + \upsilon_k + \psi Experience_i + \nu_{ijkt}, \quad (1)$$

where P_{ijkt} is the probability that individual i born into location j and currently residing in k will be sensitive to violence or crime at age t; z_i is person i's time-invariant idiosyncratic sensitivity; X_i is a vector of other individual characteristics; $f(t)$ is a function of individual i's age; μ_j and υ_k are common to all individuals for birth and current locations j and k, respectively; and $Experience_i$ indicates whether the household has directly experienced violence or crime.

The focus here is on z_i, which I interpret as an individual's intrinsic, time-invariant personality trait. The LSMS includes a module in which individual respondents were asked what they perceive to be the most challenging problems facing their local communities; crime and violence are among the possible choices. I use the dummy variable indicating whether an individual perceives these as among the most pressing local challenges as our measure of P_{ijkt}. Regressing this measure on X_i, a quadratic function of age t, dummy variables for each birthplace and current location, and $Experience_i$, I obtain the residual value of $z_i + \nu_{ijkt}$. Because simply taking this residual value directly as our measure would include significant error in variables, I use the gross variation in this value by creating a categorical variable $Sens_i$, which equals one for the top half of these values and zero for the bottom half. Because this approach involves a preliminary estimation that could create a separate error-in-variables problem, I bootstrap the full estimation using 192 bootstrap iterations. Using this approach, I control for an individual's actual experience with violence, which may be an outcome of migrant status or correlated with other unobservable characteristics.[5] The resulting measure, $Sens_i$, allows me to focus spe-

[5] Indeed, Chamarbagwala and Morán (2011) find that violence during the Guatemalan civil war caused the most exposed individuals to complete significantly fewer years of schooling. I thus control for schooling and other outcomes, more recent violence experience, and the direct effect of birth in a dangerous location.

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cifically on a person's perceptions of violence not related to her underlying income-generating capabilities or creditworthiness.

Nonetheless, there are several reasons why this sensitivity may not serve as a valid instrument for migration propensity. First, such sensitivity could still be correlated with other individual-level characteristics that I do not observe, such as risk aversion or other entrepreneurial attitudes. If this is the case, this sensitivity may in fact be correlated with the returns to capital for an individual's loan application and thus directly affect her loan approvals or rejections independently of any migration-related effects (violating the exclusion restriction). Second, the effect of this sensitivity need not be uniform given the relative variation in actual violence rates across different locations. If certain areas of the country are particularly violence ridden, we may expect to see a much stronger out-migration response for particularly sensitive individuals born into these areas than from those where violence and crime are much less prevalent. Imposing a uniform response by examining the average effect of sensitivity to crime across the sample may attenuate the role played by this sensitivity.

To address these issues, I utilize the interaction of one's sensitivity to crime with the level of actual violence and crime in an individual's birthplace as my instrument. Using this interaction allows me to control for the direct effects of being sensitive to violence on credit holdings, highlighted as potentially problematic in the first point above, as well as the direct effect of one's birthplace. My identifying assumption is that the effects on credit holdings of any other unobserved characteristics correlated with sensitivity do not vary by the relative level of violence in an individual's birthplace. The first-stage regression assessing migration propensity is the following:

$$M_{ijk} = (Sens_{ijk} \times Danger_j)\alpha + X_{ijk}\beta + Sens_{ijk}\gamma + Experience_{ijk}\lambda + \delta_j + \eta_k + \epsilon_{ijk}, \quad (2)$$

where M_{ijk} is the out-migration decision of individual i born into location j and currently residing in k, and δ_j and η_k are fixed effects for birth and current locations.[6]

To capture the threat of danger in each person's birthplace, I use the average

[6] There are several reasons why the estimate of α in eq. (2) is likely a lower bound of the true effect of violence on migration. First, I am not estimating the direct effect of location-level danger, only the differential effect on those particularly sensitive to such danger. Second, since an individual's actual experience may be a result of her migration decision, λ may suffer from reverse causation/simultaneity problems and may capture some of the effects of α. Nonetheless, my focus is on identifying an exogenous source of variation and not assessing the total effect of the average violence in a location on individual migration decisions.

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probability of experiencing violence or crime in each location, both because her own experience with violence may be endogenously determined by her migration and because her expectation of being victimized may be informed not only by her own experience but by the experience of the broader set of people in the community. In fact, individuals who reside in a crime-ridden area but have not themselves been victimized may indeed be quite fearful about their likelihood of becoming crime victims. Thus, I use the average level of violence and crime in one's birthplace in constructing the instrument for migration.[7]

My primary estimation uses linear probability models, as these are computationally straightforward in a two-stage least-squares (2SLS) setting. The equations I estimate are the following:

$$\begin{aligned} L_{ijk} &= M_{ijk}\theta + X_{ijk}\phi + Sens_{ijk}\phi + Experience_{ijk}\mu + \nu_j + \rho_k + \upsilon_{ijk}, \\ M_{ijk} &= (Sens_{ijk} \times Danger_j)\alpha + X_{ijk}\beta + Sens_{ijk}\gamma + Experience_{ijk}\lambda \\ &\quad + \delta_j + \eta_k + \epsilon_{ijk}, \end{aligned} \tag{3}$$

where L_{ijk} is a dummy variable indicating whether the individual has a loan from a bank or cooperative. The exclusion restriction I assume is

$$E((Sens_{ijk} \times Danger_j) \times \upsilon_{ijk} | X_{ijk}, Sens_{ijk}, Experience_{ijk}, D_j, D_k) = 0, \tag{4}$$

where D_j and D_k are dummy variables indicating an individual's birth and current departments. That is, the interaction of an individual's sensitivity to crime and birth in a particularly violent area affects the likelihood of having a loan only through its effects on migration (conditional on the individual's other characteristics).

There are a number of potential scenarios in which this exclusion restriction could be violated. For example, unobserved individual traits such as entrepreneurial skill could be negatively correlated with both individuals' sensitivity to violence and the selection of migrants out of particularly dangerous areas. In Section VII, I construct an indirect test of the validity of this restriction. More generally, though, I note that caution should be applied in interpreting the IV results as entirely unbiased. Rather, the estimates can be considered

[7] This approach raises a separate issue: although high violence rates in their birthplaces may indeed drive individuals to leave, they may also be correlated with other unobserved characteristics shared by individuals born in these locations. Estimation using location-level averages to explain individual-level outcomes can particularly suffer from such a "reflection problem" (Manski 1993). I follow Bertrand, Luttmer, and Mullainathan (2000), who address this problem in the context of their estimation of the social effects of welfare receipt by using heterogeneity in the impact of the network to identify its effects. I adopt a similar heterogeneity-based approach, adapting it to an IV setting in which the heterogeneous social effects are used as instruments for migration and can be reasonably assumed not to directly affect individuals' credit holdings.

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TABLE 1
SUMMARY STATISTICS

Variable	Mean	SD	Minimum	Maximum
Individual has loan from bank or cooperative	.047	.211	0	1
Migrant across department as adult	.164	.37	0	1
Violence or crime listed as primary or secondary local problem	.078	.268	0	1
Born in one of four most dangerous departments	.248	.432	0	1
High sensitivity to local violence or crime × birth in dangerous department	.053	.224	0	1
Household experienced crime in last 12 months	.142	.349	0	1
Age	44.552	15.401	14	99
Household size	5.198	2.515	1	18
Female	.178	.382	0	1
Poverty rating 2 (poor)	.355	.478	0	1
Poverty rating 3 (not poor)	.54	.498	0	1
Total individual income (monthly, 1,000s of quetzales)	1.53	9.18	0	500
Highest level of education:				
Primary	.448	.497	0	1
Middle	.145	.353	0	1
Secondary	.051	.219	0	1
Postsecondary	.005	.068	0	1
Adult education	.005	.067	0	1
Kiche	.067	.25	0	1
Qeqchi	.073	.26	0	1
Kaqchikel	.089	.285	0	1
Mam	.051	.22	0	1
Other Maya	.100	.3	0	1
Garifuna	.002	.042	0	1
Xinka	.003	.054	0	1
Nonindigenous	.615	.487	0	1

Note. Sample of 6,843 household heads. The omitted poverty rating is "extremely poor."

as highly suggestive evidence of a relationship between migrant status and credit supply.

V. Data

I use the Encuesta Nacional Sobre Condiciones de Vida (a World Bank–supported LSMS) conducted in 2000 throughout Guatemala, which surveyed individuals in 7,276 households. Table 1 presents summary statistics for these data. Guatemala is divided into 22 departments and 332 municipalities. The survey includes questions detailing in which municipality an individual was born, where she resided 5 years earlier, and how long she has lived in her current municipality. I primarily focus on interdepartment migration over a

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person's adult lifetime by comparing her birthplace with her current residence.[8] Because long-term migration is often a household decision, I limit the sample to household heads. Moreover, a number of small departments had relatively few people surveyed and did not have any individuals who listed violence or crime as the major issue there (discussed below). I eliminate these individuals from my estimation sample because the fixed-effects estimation perfectly predicts their sensitivity to crime. Doing so reduces the sample to 6,843 household heads. More than 24% of these individuals migrated across departments sometime in their lives, with approximately 16% having migrated to their current department in the previous 10 years.

The Guatemala LSMS does ask those who migrated between 1995 and 2000 to list the reason for their migration. Unfortunately, only 411 respondents in the sample of nearly 7,000 migrated over this time frame and answer this question. Of these, only 10 respondents list "violence" as the reason for their migration. A large share list familial (176) or marital (31) reasons for migration. This pattern is not surprising as the end of the Guatemalan civil war in 1996 allowed many individuals who had migrated earlier because of violence to rejoin their immediate or extended families. To identify the causes of migration prior to the end of the civil war, I make use of the social capital module of the LSMS that asked respondents, "In your opinion, what are the two principal problems in this community that you consider should be resolved in an urgent fashion?" Individuals can respond by choosing from among a list of categories, including unemployment, lack of health services, lack of education services, violence, crime, insecurity, drug addiction, alcoholism, corruption, high costs of living, too much trash, growing social differences, disintegration of the society and the family, and so forth. Approximately 7.6% of the household heads listed violence or crime among the two most urgent problems. The module also asks about individual experiences with conflict, such as whether the respondent has been a victim of assault, robbery, vandalism, land conflict, or other violence in the previous 12 months. Using this component, I can condition an individual's perceived importance of violence or crime on her experiences, as well as other covariates, as detailed in the previous section.

One might be concerned that the LSMS asks respondents about the problems in their *current* location rather than in their *birth* department, and thus their response may in fact be an outcome of their migration decision rather than

[8] Naturally, this approach does not address phenomena of return and seasonal migration but is consistent with previous analyses of migration patterns within Guatemala (Micklin 1990; Morrison 1993). In addition, for this analysis, I use information on the number of years an individual has lived in her current location to label as nonmigrants individuals who moved before they were 18.

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an exogenous predictor of this decision. In Section VII, I discuss this possibility in further detail, noting the conditions under which it could lead to a violation of the exclusion restriction and offering several tests of this possibility.

To capture the departmental danger level, I use the responses to the question about victimization in the previous 12 months to construct the share of individuals who have been victimized at the department level (using the survey's sampling weights to reflect the mean rates in the population). In order to assess the effect of violence and crime on a person's out-migration decision, it would be optimal to have time-varying data on the victimization rates to better match them to each person's birth or age. While crime reported to official authorities could offer one source of such data (e.g., annual murder rates based on police statistics), there are a wide variety of reasons that crime-reporting rates vary significantly and are often correlated with institutional and economic development (Soares 2004a, 2004b). Guatemala's war involved substantial violence that went unreported, often intentionally so, as much of this violence was carried out by government forces.[9] In the absence of reliable, time-varying data, cross-sectional survey data can capture important differences in actual violence and crime rates that are more likely to be constant over time. I use the rank order of the departments in their current violence rates from each individual's birth department, which is more likely to remain relatively constant over time. In particular, I generate a dummy variable indicating whether an individual's birth department is among the top four most violent departments (i.e., Guatemala City, Sacatepequez, Chimaltenango, and Escuintla).[10] Approximately 24% of the household heads were born in one of these departments. It is the interaction of this dummy variable and the individual's sensitivity to violence that serves as the primary instrument.

The credit section of the LSMS includes a variety of information on a household's current loan holdings. Only approximately 10% of household heads held loans of any type (the share is slightly higher if I consider all loans held by household members, not just its head). These loans were provided by a variety of sources, with 22% coming from banks (both private and state), 15% from cooperatives, 13% from individual moneylenders, and 25% from relatives or friends. I focus the analysis on the probability of having a loan from a bank or cooperative as these are the main sources of formal credit. Most of the loans

[9] In 2005, approximately 80 million documents of the disbanded National Police were uncovered, including many indicating murders and disappearances. The government of Guatemala's Human Rights Ombudsman's Office continues to process these documents (Human Rights Watch 2009).
[10] These departments were not the focus of the violence during the most intense periods of the Guatemalan civil war (1979–84), as documented by Chamarbagwala and Morán (2011), but have been the subjects of consistent violence and crime for the past several decades.

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from all sources had been held for less than 1 year, and many had relatively short-term repayment schedules typical of developing country contexts. Unfortunately, on the subject of loan applications, the LSMS asks only whether anyone in the household has applied for a loan in cash, presumably from any source, in the previous 12 months. We can surmise that most of these applications were made to formal sources rather than informal as the latter generally do not have application processes.[11]

The LSMS also asks the subset of respondents who applied for loans but were rejected what reason they were given for the rejection. While the sample of such respondents is too small to make comparisons of these reasons statistically meaningful, these reasons do provide interesting suggestive information: whereas the most common reasons cited for denying nonmigrants loan applications were "insufficient income" and "incomplete documentation," the most common reason cited for denying migrants was "other."

Finally, other individual characteristics used as covariates include the individual's age, highest education level attained, gender, ethnicity, general poverty level, monthly income in the preceding month, social capital, and household size, among others. The ethnic group categories are Kiche, Qeqchi, Kaqchikel, Mam, other Mayas, Garifuna, Xinka, and nonindigenous. The measure of social capital I use is the total number of social activities in which the individual has taken part in the past year, with 13 possibilities including collective action, solidarity, and religious groups.

VI. Results

A. First Stage

Table 2 presents the first-stage estimation of migration determinants. Column 1 shows the conditioning of one's sensitivity to violence or crime on the set of covariates using a probit estimation. Department fixed effects are included for each individual's current residence and birthplace, controlling for the mean perception of violence among individuals born and currently living in each location. Not surprisingly, individuals who had actually experienced conflict in the previous 12 months were more likely to rate violence as a key issue in the local community. Women and those less poor were also significantly more

[11] Approximately 1% of the total sample of individuals reported not having applied for loans but nonetheless hold loans from formal sources. I consider these individuals to be applicants, bringing the share of individuals who applied for loans to approximately 13%. Moreover, it is also likely that at least some individuals correctly anticipated that they would be rationed and thus never applied for loans in the first place. Thus, to the extent that we find an effect of being a migrant on loan holdings conditional on observed applications, we can surmise that this is a lower bound of the full effect of credit rationing, which would include those individuals who would otherwise be induced to apply for loans.

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TABLE 2
FIRST STAGE

	Dependent Variable	
	Violence or Crime Listed among Top Problems: Probit Estimation (1)	Migrated across Department as Adult: OLS (First-Stage) Estimation (2)
High sensitivity to local violence or crime		.3683***
		(11.23)
High sensitivity to local violence or crime × birth in dangerous department		.4929***
		(7.49)
Household experienced crime in last 12 months	.0354***	.0069
	(5.22)	(.57)
Social capital activities	−.0032*	−.0062**
	(−2.42)	(−2.91)
Age	.0012	.0038*
	(1.42)	(2.46)
Age squared	−.0000	−.0000*
	(−1.12)	(−2.14)
Household size	−.0006	.0012
	(−.47)	(.72)
Monthly income	−.0000	−.0000***
	(−.16)	(−6.26)
Highest level of education:		
Primary	.0092	.0101
	(1.40)	(1.23)
Middle	.0506***	.0255
	(4.91)	(1.75)
Secondary	.0426**	.0289
	(3.15)	(1.29)
Postsecondary	.1081**	−.5438***
	(2.80)	(−4.97)
Adult education	−.0218	−.2387
	(−.58)	(−1.94)
Poverty rating 2 (poor)	.0228	.2388***
	(1.43)	(4.10)
Poverty rating 3 (not poor)	.0518***	.2925***
	(3.45)	(4.96)
Female	.0160*	−.0222*
	(2.50)	(−2.21)
Observations	6,843	6,843
Ethnic group, current department, and birth department dummies	Y	Y

Note. Bootstrapped *z*-statistics are in parentheses. The omitted poverty rating is "extremely poor." The omitted ethnic group is Kiche.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

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likely to cite violence or crime among the top issues, as were certain ethnic groups (including several Mayan groups).

As previously noted, the residual of this regression is grouped into two categories, indicating high and low sensitivity to violence or crime. I create a dummy variable, *sensitivity to violence or crime*, which takes on a value of one for those in the high group and zero for those in the low group. Given that this adds a preliminary stage to my estimation procedure, subsequent estimations could be subject to greater uncertainty than implied by conventional standard errors. To address this, I bootstrap the subsequent estimation procedures—including the demeaning of the sensitivity variable—using 192 resampling iterations and present the bootstrapped standard errors.

In column 2 of table 2, I regress migrant status on this sensitivity, as well as its interaction with the indicator for birth in a particularly violence-ridden area. Once again, I include dummies for both departments of current residence and departments of birth to control for common unobserved characteristics among individuals living and born in each area. Notably, migration is significantly driven by both one's sensitivity to violence and the interaction of this sensitivity with actual violence indicators at the department level, with both coefficients being large, positive, and highly significant. Being older, less poor, and male is also significantly associated with the probability of having migrated during one's lifetime.

Moreover, each individual's actual crime experience does not directly affect migration. This result is particularly important because if migrants are more likely to be targeted for crime or violence in their new locations, we might be concerned that one's sensitivity to crime changes after migration. If this were the case, we would expect to see an effect of experience with violence on migration due to reverse causation; we do not find this result, suggesting that migration likely does not endogenously affect our measure of sensitivity to violence. In Section VII, I discuss several other tests of the relationship between sensitivity to violence and migration, as well as of the validity of the exclusion restriction based on this relationship.

One of this study's main aims is the identification of an exogenous, one-time motive for migration. That is, after a person who is particularly sensitive to violence or crime moves away from her particularly dangerous birthplace, she should be much less likely to move again. This would be the case if most of highly sensitive migrants born into dangerous locations move to less dangerous locations. Once a highly sensitive individual has moved to a less dangerous location, her motive for further migration would be substantially reduced.

Table 3 confirms that the instrument I construct is related to such a pattern

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TABLE 3
MIGRATION PATTERNS OF MIGRANTS BORN INTO ONE OF THE MOST DANGEROUS DEPARTMENTS

	Currently Reside in One of the Other Dangerous Departments	
Sensitivity to Violence or Crime	No	Yes
High	.61***	.39***
	(7.38)	(7.38)
Low	.25***	.75***
	(9.76)	(9.76)
Difference		.36***
		(*p*-value = .000)

Note. Sensitivity to violence or crime is conditional on individual characteristics. Bootstrapped *z*-statistics are in parentheses.
*** Significant at the .1% confidence level.

of migration. I limit the sample to individuals who were born into the most dangerous departments and who migrated away from them. Among these migrants, we find stark differences in the danger level of their destinations based on the individual's sensitivity to such danger: 75% of low-sensitivity individuals move to other dangerous locations, whereas only 39% of highly sensitive individuals do so. The large difference between these probabilities is statistically significant at the 99.9% level. These findings provide evidence that migration among highly sensitive individuals born into dangerous locations is of a different nature than of less sensitive individuals and is not likely to be a recurring phenomenon.

B. Second Stage

I next turn to the question of whether these migration determinants lead to worse credit access. Table 4 presents the results of 2SLS estimation of loan market outcomes on migrant status, instrumented with the interaction of sensitivity to violence and birth in a dangerous department. To identify the differential effect of migrant status on access to credit, I estimate the effects on loan applications separately from those on loan holdings. As column 1 of table 4 indicates, being a migrant does not cause an individual to be less likely to apply for a loan (the coefficient on being a migrant is 0.016, with a *t*-statistic of 0.16). Having greater social capital, being male, and being better educated are positively associated with the probability of having applied for a loan. This suggests that any differential effect on loan holdings for migrants is not likely to be due to differences in their demand for loans.

Column 2 of table 4 presents the effect of migrant status on loan holdings, regressing the indicator of loan holdings on migrant status and covariates in the full sample. One finds that being a migrant causes an 11.6% reduction

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TABLE 4
SECOND STAGE

	Dependent Variable	
	Applied for Loan (1)	Has Loan (2)
Migrant across department as adult	.0156	−.1163*
	(.16)	(−2.04)
High sensitivity to local violence or crime	.0088	.0453*
	(.25)	(2.08)
Household experienced crime in last 12 months	.0095	−.0151*
	(.77)	(−2.03)
Social capital activities	.0116***	.0050**
	(4.80)	(2.90)
Age	.0026	.0051***
	(1.80)	(5.91)
Age^2	−.0000**	−.0001***
	(−2.60)	(−6.25)
Household size	.0065**	.0027*
	(3.01)	(2.28)
Monthly income	−.0000	.0000
	(−.12)	(1.29)
Highest level of education:		
Primary	.0065	.0135*
	(.66)	(2.45)
Middle	.0742***	.0650***
	(5.05)	(5.52)
Secondary	.0847***	.1165***
	(3.34)	(5.43)
Postsecondary	−.0322	.0723
	(−.47)	(1.02)
Adult education	.0410	.0125
	(.73)	(.34)
Poverty rating 2 (poor)	.0070	.0324*
	(.32)	(2.17)
Poverty rating 3 (not poor)	.0255	.0614***
	(.91)	(3.35)
Female	−.0247*	−.0163**
	(−2.53)	(−2.78)
Observations	6,843	6,843
Ethnic group dummies	Y	Y
Current department dummies	Y	Y
Birth department dummies	Y	Y

Note. *z*-statistics from bootstrapped estimation are in parentheses. The omitted poverty rating is "extremely poor."
* $p < .05$.
** $p < .01$.
*** $p < .001$.

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in the unconditional probability that an individual will have a loan. With more than 16% of the population having migrated as an adult, this is an economically important effect on credit holdings.

Supporting the estimated magnitude of the effect of migration is the relatively strong first stage. The instrument is quite "relevant," and its large first-stage coefficient (0.49) and *t*-statistic (7.49) suggest that the second stage should not suffer from bias due to "weak instrument" problems highlighted by Bound, Jaeger, and Baker (1995) and Staiger and Stock (1997). Moreover, comparing the magnitude of the first-stage coefficient on the interaction of sensitivity and birth in a dangerous department with the second-stage coefficient on migration (−0.116), we find that much of the reduced-form effect of this interaction on credit (−0.034) is due to the first-stage relationship. That is, the first stage robustly identifies migration propensity, which would support the identification of even a relatively weaker relationship between migration and credit access.

Finally, it is useful to note that OLS regressions of loan holdings on migrant status (omitted for brevity) yield statistically insignificant effects. This is consistent with the confounding effects of reverse causality and omitted variables noted earlier and suggests that the 2SLS approach developed here is indeed useful in highlighting the underlying relationship between migrant status and lender supply decisions.

VII. Exclusion Restriction Checks

As previously mentioned, the exclusion restriction I assume could be violated under a number of scenarios. First, measurement issues (including timing) in the instrument I use could make it endogenously related to migration and thus potentially correlated with the error term in the second stage. Second, because I rely on a linear interaction term as my instrument, my estimates could be biased if there are nonlinearities in the direct effects of the individual variables. Third, unobserved individual characteristics could be correlated with individual sensitivity to violence, birth in a dangerous department, and lender supply decisions.

One way to assess the extent of these biases would be to conduct overidentification tests comparing the estimates found when using different subsets of instruments. This is not possible in this case, as I utilize a just-identified IV approach. However, one can check whether patterns in the data are consistent with these potential sources of bias. I conduct several such checks, discussed below. While the findings largely support the exclusion restriction, I cannot rule out the possibility that omitted variables play at least some role in these estimates. The results should thus be interpreted as suggesting that migrants

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TABLE 5
EXPERIENCE WITH VIOLENCE, MIGRANT STATUS, AND BIRTH IN A DANGEROUS LOCATION

	Dependent Variable: Household Experienced Violence	
	(1)	(2)[a]
Among top four most dangerous departments	.0848***	−.0211
	(7.40)	(−.34)
Migrant across department as adult	.0672***	.0074
	(5.07)	(.42)
Birth department among most dangerous × migrant	−.0299	.0549
	(−.90)	(1.57)
Observations	6,843	6,843

[a] Covariates included are age, gender, education, ethnicity, household size, and current and birth departments. Bootstrapped *z*-statistics are in parentheses.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

face at least some barriers to credit that are causally related to their status as migrants, though the precise effect could be smaller in magnitude than the estimates herein.

A. Timing Issues

As previously noted, the data I use refer to perceptions of violence and crime in respondents' current communities rather than in their birth communities. This is not ideal as these perceptions could be endogenously related to migration as well as to other features of the current location. If the endogenous component of these perceptions is correlated with both birth in a dangerous location and the probability that an individual receives a loan after applying, the exclusion restriction could be violated. There are several ways in which this could arise: First, migrants may be more likely to be targeted for violence or crime within their communities, raising their sensitivity to these issues. If migrants born into dangerous communities are particularly likely to be targeted, the interaction of this birthplace danger and sensitivity could indeed be endogenously determined by migration. I address this in several ways: in addition to directly controlling for the household's actual experience with violence and crime in all of the estimations, I test whether migrants born into one of the most dangerous departments are indeed differentially more likely to experience violence or crime. Table 5 presents the results of an OLS regression of the dummy variable indicating whether the household has experienced any violence or crime on migrant status, birth in a dangerous department, and the interaction of these terms. Regardless of whether other covariates are included, the interaction of migrant status and birthplace danger does not

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TABLE 6
SENSITIVITY TO VIOLENCE AND TIMING OF MIGRATION

	Sensitivity to Violence or Crime	
	Low	High
Time of Last Migration	(1)	(2)
Less than 12 years ago	.160***	.840***
	(20.33)	(20.33)
At least 12 years ago	.132***	.868***
	(24.79)	(24.79)
Difference		−.028
		(*p*-value = .137)

Note. Sensitivity to violence or crime is conditional on individual characteristics. Bootstrapped *z*-statistics are in parentheses. The sample is migrants born into one of the four most dangerous departments.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

appear to be correlated with heightened levels of actual violence experienced by the household. This suggests that the extent to which migrants are indeed targeted for violence does not vary with the danger level of their birthplace, and the resulting effect on sensitivity to violence would similarly not be correlated with the danger level of their birthplace.

A second potential reason that my measure of sensitivity to violence or crime may be endogenous may be its correlation with other current characteristics, both observed and unobserved. Again, my identification strategy depends on this measure being a time-invariant feature of the individual's personality. If this measure instead reflects current conditions that are correlated with both the danger level in one's birthplace and the probability of receiving a loan, the exclusion restriction would be violated. I address this in several ways: First, I control for the observed individual characteristics. Second, I use fixed effects for the current location to control for common characteristics in that location. Finally, I devise a test to assess whether the sensitivity measure appears to vary over the length of time migrants have resided in their destination. If the sensitivity measure I use indeed includes a time-varying component that is correlated with migration, one would expect that more recent migrants would be more sensitive to violence than less recent migrants.[12]

Table 6 shows the results of this test, indicating that the difference between more and less recent migrants in their sensitivity to violence is not significant. In fact, the difference suggests that more recent migrants are less sensitive to violence or crime. This test indicates that, after controlling for a variety of

[12] This timing is independent of the migrant's age, which was directly controlled for in the formulation of $Sens_i$.

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individual characteristics, direct experience with violence, and locational averages, the residualized measure of sensitivity to violence and crime I develop reflects an individual's time-invariant attitude toward these features. In Section VI, I described a similar test that assessed whether those individuals born into dangerous locations who are most sensitive to violence are more likely to have moved into less dangerous locations than those who are less sensitive (see table 3). Again, if this sensitivity is indeed correlated with current departmental danger, one would expect the most sensitive migrants to be more likely to reside in more dangerous locations rather than less.

Taken together, these tests suggest that the timing of the LSMS question regarding respondents' primary concerns in their current communities, although not ideal, does not appear to lead to a violation of the exclusion restriction.

B. Misspecification Issues

A second potential threat to my exclusion restriction is that using an interaction term as an instrument could render my estimates susceptible to nonlinearities in the effects of the individual variables. Although I address the issue of demand factors in Section VI, this section checks that model misspecification in its treatment of demand does not lead to a violation of the exclusion restriction. As an example, consider the following model:

$$L_{ijk} = D_{ijk} \times S_{ijk} + \epsilon_{ijk}, \tag{5}$$

$$D_{ijk} = \alpha_1 + \alpha_2 Sens_{ijk} + \alpha_3 X_{ijk} + \alpha_4 Danger_i + \alpha_5 \nu_j + \alpha_6 \rho_k, \tag{6}$$

$$S_{ijk} = \beta_1 + \beta_2 Danger_j + \beta_3 M_{ijk} + \beta_4 X_{ijk} + \beta_5 Danger_i + \beta_6 \eta_k, \tag{7}$$

where D_{ijk} is the probability that individual i born in j and currently living in k applied for a loan, and S_{ijk} is the probability that a lender approved a loan conditional on an individual applying. Here, a person's sensitivity to local violence affects her demand for loans. Moreover, lenders know and use the danger level of a person's birthplace as a factor in approving her loan. In this model, having a loan is a nonlinear function of these variables. In this case, we can rewrite this model as

$$L_{ijk} = \alpha_1\beta_1 + \alpha_1\beta_2 Danger_j + \alpha_1\beta_3 M_{ijk} + \cdots + \alpha_2\beta_2 Sens_{ijk} \times Danger_j + \cdots. \tag{8}$$

In this model, being a migrant may affect whether we observe her holding any loans (i.e., if $\alpha_1\beta_3 > 0$). However, we may not be able to use the main

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instrument ($Sens_{ijk} \times Danger_j$) in an IV estimation because it may directly enter into the main regression (i.e., if $\alpha_2\beta_2 > 0$). Moreover, one cannot directly test this condition as the resulting specification would be indistinguishable from my existing reduced-form specification. I can, however, assess whether $\alpha_1\beta_2 > 0$. More intuitively, this is akin to testing whether, under this alternative model, lenders do differentiate their borrowers on the basis of the danger level in individuals' birthplaces (i.e., $\beta_2 > 0$). It offers an indirect test of the exclusion restriction.

To implement this test, I reestimate the reduced-form specification and test whether the fixed effects for each of the four most dangerous birth departments are jointly significant. The *p*-value on this *F*-test is only .381, suggesting that individuals from these departments do not appear to be differentially affected. In other words, there is no direct effect of being born in a dangerous location on the probability of holding a loan. As such, we can be more confident that the effect of birthplace danger on credit occurs through the migration decision and not through a nonlinear interaction of demand and supply decisions.

Finally, as one additional specification check, I assess whether there is any evidence that interaction terms between $Sens_{ijk}$ or $Danger_j$ and any of the other covariates enter into the main regression. To do so, I sequentially include the interactions of $Sens_{ijk}$ with X_{ijk} in the second-stage regression, as well as the interactions of $Danger_j$ and X_{ijk} (X_{ijk} includes age, age squared, household size, gender, poverty rating, education level dummies, and ethnicity dummies). Notably, none of these interaction terms yields a significant coefficient.[13] Thus, even if one believes that $Sens_{ijk}$ or $Danger_j$ enters the main regression nonlinearly, the higher-order terms of these variables are not correlated with any of the covariates. It seems relatively improbable that the interaction of $Sens_{ijk}$ and $Danger_j$ is the only interaction that is correlated with the higher-order terms of either of these variables.

VIII. Alternative Explanations

I next assess whether the primary findings could be driven by alternative explanations rather than my main claim that weak contract enforcement and information sharing led to greater rationing of loans to migrants. In particular, I examine whether moral hazard, adverse selection, or differential urban-rural migration is the primary cause leading us to observe fewer loans to migrants.

[13] When all the interactions are included jointly in one main regression, the *F*-statistic testing whether they are jointly different from zero is 1.48 and is not significant.

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A. Moral Hazard

As previously noted, credit rationing has often been linked to issues of involuntary default because of informational asymmetries. A notable example is moral hazard, which could particularly affect migrants if there is greater uncertainty about their actions or production choices. Informal lenders often rely on social mechanisms to monitor individuals' behavior. If moral hazard is the core factor causing rationing of loans to migrants, informal lenders would be particularly susceptible to this issue. Banks and other formal lenders, on the other hand, may not find social monitoring of moral hazard as crucial in their lending policies toward migrants. I therefore assess whether rationing of loans to migrants is particularly heightened among informal lenders relative to formal lenders. Table 7 shows the results of running the 2SLS estimation while considering dependent variables capturing different loan sources. Column 1 repeats the baseline results from table 4. Column 2 presents an identical regression in which the dependent variable is now a dummy denoting whether the individual has a loan from a family member, friend, or individual. Notably, I find almost no effect of migration on informal lending, suggesting that moral hazard is unlikely to be driving the phenomenon of reduced lending to migrants.

B. Adverse Selection

Rationing due to involuntary default could also be driven by adverse selection issues. A credit market could exhibit greater rationing of loans to migrants if lenders "know less" about individual migrants' expected returns than they do about individual natives. As previously noted in Section II, different risk formulations have been shown to lead to overlending rather than to rationing, and the equilibrium outcome under adverse selection depends on the distributions of the different parameters. Nonetheless, I examine whether the main effect of being a migrant on credit is driven by lender uncertainty about a borrower's income. All else equal, potential borrowers whose income comes primarily from less formal or verifiable sources should be less likely to receive loans. The LSMS includes detailed information on income sources; I calculate the share coming from "other sources," composed of transfers from other community members and organizations (primarily churches), as well as remittances from both other parts of the country and abroad. This calculation excludes government transfers for retirees, orphans, and those receiving food support.

From a lender's perspective, these "other sources" of income are likely to be less verifiable or more opaque than labor income or government transfers. Other factors may also affect a lender's uncertainty about a potential borrower's income, including the underlying volatility in the person's labor income.

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TABLE 7
ALTERNATIVE EXPLANATIONS: MORAL HAZARD AND ADVERSE SELECTION

	Dependent Variable		
	Has Formal Loan: Baseline (1)	Has Informal Loan: Moral Hazard (2)	Has Formal Loan: Adverse Selection (3)
Migrant across department as adult	−.1163*	−.0303	−.1307*
	(−2.04)	(−.49)	(−2.04)
High sensitivity to local violence or crime	.0453*	.0149	.0483*
	(2.08)	(.68)	(2.15)
Share income from "other" sources			−.0006
			(−1.04)
Household experienced crime in last 12 months	−.0151*	−.0013	−.0150*
	(−2.03)	(−.18)	(−2.09)
Social capital activities	.0050**	.0026	.0048**
	(2.90)	(1.94)	(3.01)
Age	.0051***	−.0017	.0052***
	(5.91)	(−1.80)	(5.68)
Age^2	−.0001***	.0000	−.0001***
	(−6.25)	(.80)	(−6.08)
Household size	.0027*	−.0000	.0028*
	(2.28)	(−.04)	(2.45)
Monthly income	.0000	−.0000	.0000
	(1.29)	(−.95)	(1.23)
Female	−.0163**	−.0073	−.0169*
	(−2.78)	(−1.11)	(−2.54)
Observations	6,843	6,843	6,843
Age, age^2 controls	Y	Y	Y
Poverty category dummies	Y	Y	Y
Education dummies	Y	Y	Y
Ethnic group dummies	Y	Y	Y
Current department dummies	Y	Y	Y
Birth department dummies	Y	Y	Y

Note. Bootstrapped *z*-statistics are in parentheses.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

Nonetheless, if the opacity of an individual's income matters, we should observe greater rationing among individuals with larger shares of relatively opaque income. If migrant status is in fact proxying for greater income opacity, including the share of income from less verifiable sources (while controlling for the total level of income) should reduce the negative coefficient on migrant status. As column 3 of table 7 shows, the share of income from these other sources is negatively related to the likelihood of having a loan, although this effect is not statistically significant.[14] Nonetheless, adverse selection due to the

[14] In other specifications not shown, I include higher-order polynomials of income to ensure that

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TABLE 8
DIFFERENCES IN RURAL-URBAN MIGRATION?

	Destination	
	Urban	Rural
Born into dangerous department × high sensitivity to violence:		
= 1	.563	.437
	(10.49)	(10.49)
= 0	.577	.423
	(38.15)	(38.15)
Difference		.014
		(*p*-value = .801)

Note. The subsample is all migrants. Bootstrapped *z*-statistics are in parentheses.

opacity of a borrower's income, here captured by the share of income from relatively less verifiable sources, does not appear to explain credit rationing to migrants.

C. Urban-Rural Migration

Another alternative explanation of the results may be that the instrument I use explains specific migration paths particularly well. For example, one might suspect that individuals who are particularly concerned about violence and crime and who are born into particularly dangerous locations may be more likely to move to rural areas, where the threat of violence and crime may be somewhat lower and credit demand may be lower. There are several reasons why such differential migration paths are not likely to drive the results. Table 8 shows that this is not the case. Individuals for whom $Sens_{ijk} \times Danger_j = 1$ are in fact no more likely to reside in a rural location than those individuals for whom the instrument equals zero.

IX. Robustness Checks

I next assess whether the main results are robust to several potential econometric issues: sampling weight heterogeneity and endogenous covariates. The primary results largely survive these sensitivity checks.

A. Sampling Weights

Complex survey designs often lead some individuals or households to be included in a sample with greater probability than would be the case in pure random sampling. Whether one should account for this nonrandom sampling in regressions depends on the goals and nature of the estimation: if the primary

the pure income effect of community support and remittances is being adequately controlled for. These do not affect the results.

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purpose is descriptive, such as examining the mean of one variable conditional on others, or if one is interested in making inferences about the full population, sampling weights should be incorporated (Deaton 1997). However, if, as in my case, the purpose of the regression is limited to assessing variable relationships within the sample, the issue of survey design is primarily one of heterogeneity in the effects (whether the effects are correlated with the sampling weights). The classic econometric argument against the weighted estimator is that it will be less efficient and have larger standard errors than the unweighted estimator when the strata are not homogeneous; it will not necessarily be superior in terms of consistency. Following Deaton, I conduct a Hausman-type specification test assessing whether the key effects vary with the sampling weights. That is, I estimate the following reduced-form specification:

$$L_{ijk} = (Sens_{ijk} \times Violence_j)\alpha + W \times (Sens_{ijk} \times Violence_j)\psi + Sens_{ijk}\gamma + W \times Sens_{ijk}\varsigma + Violence_i\lambda + X_i\beta + \delta_j + \eta_k + \epsilon_{ijk},$$

where W is the matrix of sampling weights. Table 9 presents these results. One can use an F-statistic to test the null hypothesis that $[\psi, \varsigma] = [0, 0]$, obtaining a p-value of .95. The coefficient on $Sens_{ijk} \times Violence_j$ does not change substantially when these interaction terms are included, although the standard error is larger and no longer significant. This is not surprising given the aforementioned efficiency trade-off in using the weighted estimator. The results suggest that relying on the unweighted estimator is preferred, as the heterogeneity in the effects of my instrument does not appear to be correlated with the sampling weights.

B. Endogenous Covariates

Although my primary IV estimation addresses the reverse causality of credit's effects on migration, it includes a number of control variables that may also endogenously affect migration. It is important to include these control variables because they may otherwise confound the effect of migration; if, for example, individuals who migrate are less poor and poverty is negatively associated with access to credit (as is the case in Guatemala), not including some measure of poverty in the estimation would bias upward the estimated effect of migration on credit. One expects the effect of migration on access to credit to be smaller in the presence of endogenous variables that might be positively correlated with a household's ability to migrate and with access to credit. If dropping these covariates substantially increases the magnitude of the migrant coefficient, we might suspect that these variables are playing a confounding role. I assess the extent to which any one of these endogenously determined covariates affects the primary results by sequentially dropping each covariate from the 2SLS

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TABLE 9

TEST OF CORRELATION BETWEEN MAIN EFFECTS AND SAMPLING WEIGHTS, OLS (REDUCED FORM)

	Dependent Variable: Individual Has Loan	
	Baseline Estimation (1)	Estimation with Sampling Weights (2)
High sensitivity to local violence or crime	.0163	.0154
	(1.51)	(1.33)
High sensitivity to local violence or crime × birth in dangerous department	−.0340*	−.0316
	(−2.03)	(−1.41)
Weighted sensitivity to local violence[a]		6.0102
		(.23)
Weighted sensitivity × birth in dangerous department[a]		−19.5234
		(−.19)
Household experienced crime in last 12 months	−.0151^{+}	−.0151^{+}
	(−1.96)	(−1.95)
Social capital activities	.0058***	.0058***
	(3.96)	(3.68)
Age	.0044***	.0044***
	(5.73)	(5.25)
Age^2	−.0000***	−.0000***
	(−6.18)	(−5.81)
Household size	.0023^{+}	.0023^{+}
	(1.72)	(1.77)
Monthly income	.0000^{+}	.0000^{+}
	(1.69)	(1.75)
Poverty rating 2 (poor)	.0141^{+}	.0141
	(1.69)	(1.55)
Poverty rating 3 (not poor)	.0369***	.0369***
	(3.55)	(3.58)
Female	−.0132*	−.0131*
	(−2.38)	(−2.26)
Observations	6,843	6,843
Age, age^2 controls	Y	Y
Education dummies	Y	Y
Ethnic group dummies	Y	Y
Current department dummies	Y	Y
Birth dept dummies	Y	Y

Note. Bootstrapped *z*-statistics are in parentheses. The omitted poverty rating is "extremely poor."

[a] Joint p-value = .9487.

$^{+}$ $p < .1$.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

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TABLE 10
DROPPING COVARIATES

	Dependent Variable: Individual Has Loan: Dropped Covariate			
	Baseline (1)	Household Experienced Crime (2)	Household Size (3)	Poverty + Income (4)
Migrant across department as adult	−.1163*	−.1170^{+}	−.1213^{+}	−.0904
	(−2.04)	(−1.95)	(−1.86)	(−1.52)
Household experienced crime in last 12 months	−.0151*		−.0143^{+}	−.0103
	(−2.03)		(−1.82)	(−1.46)
Social capital activities	.0050**	.0048**	.0052**	.0061***
	(2.90)	(2.72)	(3.13)	(3.78)
Age	.0051***	.0051***	.0058***	.0056***
	(5.91)	(5.47)	(6.54)	(5.80)
Age^2	−.0001***	−.0001***	−.0001***	−.0001***
	(−6.25)	(−5.90)	(−6.96)	(−6.28)
Household size	.0027*	.0026*		−.0000
	(2.28)	(2.11)		(−.04)
Monthly income	.0000	.0000	.0000	
	(1.29)	(1.43)	(1.52)	
Poverty rating 2 (poor)	.0324*	.0313*	.0295*	
	(2.17)	(2.26)	(2.20)	
Poverty rating 3 (not poor)	.0614***	.0594**	.0537***	
	(3.35)	(3.26)	(3.30)	
Female	−.0163**	−.0165**	−.0186**	−.0142*
	(−2.78)	(−2.58)	(−3.28)	(−2.18)
Observations	6,843	6,843	6,843	6,843
Education dummies	Y	Y	Y	Y
Ethnic group dummies	Y	Y	Y	Y
Current department dummies	Y	Y	Y	Y
Birth department dummies	Y	Y	Y	Y

Note. Bootstrapped *z*-statistics are in parentheses. The omitted poverty rating is "extremely poor."
$^{+}$ $p < .1$.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

regression. The results, presented in table 10, suggest that omitting the household's size and experience with crime does not appear to substantially alter the results by more than 1 percentage point (though the standard errors also expand slightly, and these coefficients become significant at the 10% level). Dropping the covariates on a household's general poverty level and the household head's income in the preceding month reduces the magnitude of the migrant coefficient from 0.116 to 0.090, but its standard error remains almost unchanged. As a result, the *z*-statistic on the coefficient is 1.52. While this is a curious change, it suggests that including a household's poverty and income levels as covariates does not confound the effect of migration on access to credit in my specifications.

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X. Conclusions

My theoretical model illustrates that migrants who move because of a random shock can experience credit rationing, even if these migrants would choose to stay in their new locations and repay their loans. This model is consistent with empirical results suggesting that migrants who move for reasons that are largely uncorrelated with credit demand or creditworthiness are nonetheless substantially less likely to receive credit from formal lenders. I find that migrants are 12% less likely to have a loan, although they are no less likely to have applied for one. Since only approximately 13.4% of household heads have applied for loans, these effects are very large and suggest that being a migrant is associated with a dramatic reduction in the likelihood of obtaining a loan when applying for one.

This article adopts a quasi-experimental strategy to identify the effect of individual migration on access to credit, using the broad thematic coverage of the World Bank LSMS to observe a key migration factor that is not directly observed by lenders. This approach certainly has limitations, among them the reliance on a just-identified estimation that does not easily lend itself to tests of the exclusion restriction. My IV approach cannot rule out all biases in these estimates from omitted variables correlated with violence patterns, sensitivity, and credit supply, and the results should be interpreted cautiously. Nonetheless, these findings do provide suggestive evidence that migrant status can be associated with informational barriers to credit access. These results do not necessarily indicate that the returns to migration are negative on net. Studies of both macroeconomic returns to liberties such as residential freedoms (BenYishay and Betancourt 2010) and individual gains from migration (Bryan et al. 2010; Clemens 2010) highlight the social and personal benefits from migration. I find that migration can induce informational challenges that can inhibit lenders from making loans to creditworthy individuals; the gains from migration could thus be even larger in settings with more effective information sharing and contract enforcement.

Strengthening contract enforcement and creditor rights poses challenges. Even in the context of weak enforcement of debt contracts, information sharing among lenders can be used to incentivize borrowers to repay their loans. In 2001, with the support of the World Bank Group, TransUnion opened the first credit bureau in Guatemala. Nonetheless, as of 2009, only approximately 28% of the country's adult population was covered by the bureau (as reported by the International Finance Corporation's *Doing Business* website, http://www.doingbusiness.org). New investments in information-sharing infrastructure in other developing countries are being pursued with similar fanfare, but the implementation of these systems remains uneven and challenging. This

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research suggests that further attention by policy makers should be devoted to the design and implementation of these systems.

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